PRELIMINARY-PLEASE DO NOT QUOTE WITHOUT PERMISSION

Did Small-Group Health Insurance Reform Affect the Price and Availability of Health Benefits?

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Abstract

Although states have taken strong measures to improve the small-group health insurance market, we know very little about their impact on small employers. I use two recently completed surveys of employer provided health insurance to investigate the effects that state small-group insurance reforms such as pricing and issuing restrictions have had on a small employer's decision to offer health insurance, the price of health insurance to the employer and to the worker, the coverage, take-up and eligibility rates, and whether the impact varies by medical risk levels. I estimate the impact of reform on small firms by using large firms in the same states as well as small and large firms in states that do not reform as comparison groups. I find evidence that stringent reforms increased premiums and employer contributions, while decreasing the rate of coverage, take-up and eligibility among small firms. However, reforms do not appear to affect a small employer's decision to offer coverage. Further analysis suggests that low-risk firms were hurt more than high-risk firms, and that reforms decreased the incidence of costly underwriting practices.

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I. Introduction

According to the U.S. Census Bureau, over 44 million Americans were without health insurance in 1998.[2] This number appears to have increased substantially during this decade despite state and federal attempts to reverse the trend. One of the most prominent ways that states have tried to attacked this problem is through small-group health insurance reform. Although employer provision is the main vehicle of medical coverage for working-age Americans, small businesses consistently display a lower propensity to insure their workers.[3] Following the collapse of national health reform earlier this decade, the impetus to improve conditions in the small-group market shifted to the state level. States revised the rules governing the small-group market in a number of ways, and as this decade draws to a close it is important to ask what the relative successes and failures of their efforts have been.

From 1990 to 1996, almost every state enacted some combination of reform designed to prevent insurers from 'cherry picking', that is, from accepting as clients firms with low-risk workers while excluding firms with high-risk workers from coverage.[4] Despite the popularity of small-group legislation among state lawmakers, we know little about the consequences for small employers.[5] Theoretically, reforms preventing insurers from discerning between groups by their risk level may have distributional consequences. Premiums will likely rise for the low-risk groups, while they may fall for high-risk groups. However, the average price of health insurance may rise as insurers raise premiums to offset the increased costs imposed by adverse selection. Price

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[2]Census Bureau Press Release, October 4th, 1999 (www.census.gov/Press-Release/www/1999/cb99-189.html

[3]Of all firms with more than 100 workers, 96% offer health insurance to their workers in 1993. For firms with fewer than 10 employees, this number is 33%. (NCHS, 1997).

[4]In 1996 Congress enacted a subset of the existing state legislation through the Health Insurance Portability and Accountability Act that became effective in July 1997. Once future waves of the employer surveys used here are available, I intend to examine the impact of this federal legislation as well.

[5]In a previous paper (Simon, 1999b) I use the March CPS from 1992 to 1997 to investigate the impact of reform on the probability that a small-firm full-time worker received employer coverage. I find some evidence that the effect of reform has not been beneficial. Several other studies use smaller data sets (and in some cases, methods that do not necessarily imply causality) to address the impact of small-group reform on insurance availability and arrive at mixed conclusions.

changes could lead to other consequences, such as changes in the required employee contribution towards the premium, and changes in employee eligibility or take-up of coverage. They could also influence the employer's decision to offer health insurance at all.

On the other hand, these policies might also lower costs by encouraging pooling and eliminating costly underwriting methods. Experience rating entails administering questionnaires and investigating health histories of employees, and forcing all insurers to abstain from these practices may reduce their costs which may lead to lower premiums overall. Have these reforms made health insurance more affordable? Have they led to improved coverage by small-employers? The political atmosphere is once again ripe for health insurance reform and there is a great need for more research into these questions.

The health insurance data on employers best suited to answer questions about the impact of reform on premiums and availability are the recently completed 1996 Meidcal Expenditure Panel Survey Insurance Component, and the 1993 National Employer Health Insurance Survey. Both are protected by federal guarantees of confidentiality and are stored at separate locations. Although I have access to both surveys, I need to use a unique method of analysis to conduct this study since raw data cannot be transferred from one location to another. I am able to conduct the equivalent of regression analysis using matrix algebra, a technique that is explained later in this paper.

In a previous study (Simon, 1999b), using data on over 220,000 full-time workers from the March Current Population Surveys of 1992-1997, I investigate whether small-group health insurance reforms affected the probability that a full-time small-firm worker receives health insurance from his/her employer. I find that stringent small-group insurance reform reduces coverage by about two percentage points for the average worker in a small firm. Further investigation reveals that the laws affected those that may be termed low-risk workers more adversely than the average worker, and had a statistically insignificant but positive effect on the high-risk workers. These results are consistent with theoretical predictions drawn from simple models of adverse selection.

This paper extends my previous work. Here I examine the factors that lead to this drop in coverage. Did reforms increase the costs borne by the firms or by the workers? Did they reduce

the rate at which firms offer workers coverage, or did they rather only lower the rate at which workers accepted coverage? In Section II I briefly review the existing literature on the effects of health insurance reform and the adverse selection issue associated with it. Section III describes the data, including the data collection effort that recorded and codified state health insurance reforms between 1990 and 1996. Section IV describes the models I estimate and a unique method of conducting regression analysis when confidentiality restrictions do not allow the outcome of interest (the price of insurance, the coverage rates etc.) and the main independent variable of interest (state health care reforms) to be linked at the micro level. Section V described the results and Section VI concludes.

II Literature Review

The literature relevant to this chapter can be split along two dimensions. One strand of literature that guides this study relates to adverse selection. The model of adverse selection most often referred to in the health insurance literature is Rothschild and Stiglitz (1976). According to this framework, one would expect that when reforms prevent insurers from discriminating between high and low-risk clients, insurers will set a price that may be higher than the price offered to low-risk groups absent reforms but lower than that previously offered to high-risk groups. If these new prices cause low-risk groups to leave the market while encouraging more high-risk clients to enter, insurers may increase premiums further. It follows from this reasoning that reforms may cause a premium increase that is larger for a low-risk group than for the average firm, and a smaller increase for a high-risk group. As a result, reform may also cause a corresponding larger decline in availability for low-risk firms.

The other body of literature which this study fits into concerns the impact of small-group reform. While many recent studies inquire about the effectiveness of state small-group regulations, almost no evidence exists on how reforms have affected health insurance premiums or employee contributions because of the lack of suitable data.[6] Several factors make the causal impact of

[6] The only known exception is descriptive data on small employers in California between 1993 and 1995 (Buchmueller and Jensen, 1997). This study showed that average premiums decrease after reform, although the authors caution that other things such as the increased penetration of managed care occurring during

reform on other outcomes such as the insurance coverage rate difficult to estimate. One approach taken in the literature is to frame this as a natural experiment, but it is quite possible that the first states to reform enjoyed better health insurance outcomes to start with.[7] An additional estimation complications arises from the possibility that small-group reform occurred at the same time as other events in a state's insurance market (Buchmueller and Jensen, 1997), thus a simple comparison of outcomes in a state over time may not be adequate to isolate the impact of reform. A lack of reliable information on the timing and nature of state small-group reform has lead to differences in coding in the existing literature, often making the analyses not comparable to each other.

To date studies have looked at the impact of reform on individuals or state-averages in terms of the offers and receipts of health insurance, and the ability to move between jobs. These include Simon (1999b); Buchmueller and DiNardo (1999); Sloan and Connover (1998); Kapur (1998); Monheit and Schone (1999); Marsteller et. al. (1997); and Zuckerman and Rajan (1999). In Simon (1999b) I discuss these studies in more detail, and conclude that there is no consensus regarding whether reforms are beneficial or detrimental at the individual level.

In contrast, employer-level studies are relatively scarce and reflect the lack of health-insurance data on employers available to researchers. Two previous studies investigate the impact of small-group reform using national employer-level data. Hing and Jensen (1999) use 1993 data on employers to compare the availability of health insurance among small firms in reform states against non-reform states. The outcomes examined are whether the firm offers health insurance, whether the plan is subject to an underwriting practice known as enrollee-exclusion,[8] and the fraction of workers who enroll in an employer's health plan. The authors find that reforms may

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there two years could also be responsible for the decline in health insurance costs.

[7] Figures in Simon (1999b), regression results in Stream (1997) and a comparison of tabulations from the Employee Benefit Research Institute in Hing and Jensen (1999) suggest that states undertaking early reform may be different from those that do not in terms of outcome measures.

[8] Enrollee-exclusion refers to the case when an insurer is able to prevent high-risk employees from enrolling in the health plan.

have improved coverage, but do not increase the proportion of workers enrolled in an employer's plan and do not decrease the incidence of enrollee-exclusion. Jensen and Morrissey (1999) study effects of small-group reform (using separate indicators for guaranteed issue, guaranteed renewal, portability of coverage, pre-existing conditions limits and bare-bones plans) on a small-employer's decision to offer insurance between during the first half of this decade. This study finds that overall, reform had little impact on an employers decision to offer coverage. All but reforms easing the portability of health insurance coverage between jobs display positive insignificant coefficients, while one (pre-existing conditions limits) carries a positive coefficient that is statistically significantly different from zero.

The primary contribution of this study is that it provides the first econometric evidence on the impact of small-group reform on the cost of health insurance, both to the employer and the employee. Prior studies have done much to advance our understanding of small-group reform on other outcomes of interest, yet there is room for improvement. The need for further research using data covering multiple years and more careful coding of state reforms is pointed out by Hing and Jensen (1999).

"There is a second explanation for our findings, however. Maybe states that acted earlier with full reforms were those that had better functioning small group markets to begin with. If so, the reform "effects" we reported may partly reflect these fundamental differences across markets, and we have likely overestimated the long-term benefits of reform. Unfortunately, as our data consists of firms observed all at the same point in time (1993), they cannot discern between these two possibilities. " .."..in classifying reform states, we relied heavily on secondary sources which may have contained errors." (P.703)

I build upon existing research by using a larger employer data set spanning several years that allows for over-time and within-state comparisons. Due to the nature of the available data and to avoid multicollinearity problems, previous analysis using employer data does not account for state-level differences in the outcomes of interest, perhaps making the impact of reform harder to detect. My investigation of over two dozen secondary reports showed a number of discrepancies which I document in Simon (1999a). Through a comprehensive investigation into the state legislative history of small-group reform, I am able to break the traditional reliance on

secondary sources.

III. Data

Many believed that by the late 1980s practices in the insurance market for small employers had reached a stage where prices were extremely volatile and rigorous underwriting rules prevented all but the healthiest from obtaining coverage. According to a leading small-firm insurer at that time, "Insurability is determined by many factors, including the nature of the business.., and most importantly, the good health of all employees and dependents."[9]

Because many viewed the prevailing conditions in the small-group health insurance market as detrimental, states adopted five basic types of small-group regulations in the 1990s. These are rating reforms, guaranteed issue, guaranteed renewal, pre-existing condition exclusion limits and portability laws. Rating reforms restrict the insurers' ability to use health related factors in setting premiums, while guaranteed issue and renewal provisions require insurers to sell/renew policies to all who apply. The last two laws shorten the time during which insurers can refuse to cover illnesses existing prior to the start of a policy and increase the ease with which workers may change jobs and retain health insurance coverage. (See Hall, 1994; Blumberg and Nichols, 1998; and Simon, 1999b for further details about these reforms.) Since the most restrictive policy would be to require insurers to sell to all comers while preventing them from pricing fully according to risk, I refer to states with both guaranteed issue and rating reforms as having 'full-reform', to states with rating reforms without guaranteed issue laws as having 'partial-reform', and to other states as having no reform. Although the versions of regulations adopted by various states differ somewhat, most legislators followed model laws written by organizations such as the National Association of Insurance Commissioners.

Data gathering for coding state reforms between 1990 and 1996 started in the Spring of 1997. I collected over 20 secondary sources reporting on small-group health insurance reform and recorded the information provided about each state from each report (the list of publications and detailed comparisons are in Simon, 1999a).These are mostly reports complied by non-profit or

[9]This quote is taken from Zellers, McLaughlin and Frick (1992), p.174.

government entities, and often ones describing early reform adoption were out of print and could only be obtained through inter-library loan. Once discrepancies became evident, I began contacting state insurance departments directly to solicit their opinions. I mailed letters to each state's health insurance regulation contact (names obtained from a list maintained by the National Association of Insurance Commissioners) asking about reforms effective in that state. I also listed the information I had gathered so far from secondary sources about their state in these letters.

After receiving responses from 36 states, I read through each state's insurance code at the Library of Congress to study the small-group insurance legislation first-hand. When only the newer versions of legislation could be found in the statutes, I went back to the actual bills to see what the effective regulations had been during the earlier years. This exercise together with the wealth of information provided by states and secondary sources helped me establish the timing and nature of reform more precisely. In certain instances, ambiguities in state legislative materials made judgement calls necessary pending responses to inquiries sent to particular state contacts. This background research resulted in a database that I intend to improve and extend to future years.

The health insurance data for this study come from two newly-available nationally-representative employer surveys that constitute a substantial improvement over existing data sources. The Medical Expenditure Panel Survey Insurance Component List Sample (MEPSIC) and the National Employer Health Insurance Survey (NEHIS) were conducted by federal agencies in 1994 and 1997 respectively to collect information about health insurance provision and other employer characteristics during the previous year for 34,604 and 23,000 private employers respectively.[10] These two surveys were undertaken by the federal government to produce data sets much larger than most existing private sector surveys on employer provided health insurance. Larger employer-level surveys have also been funded by the Robert Wood Johnson Foundation (RWJF) and conducted by RAND (e.g. the 1993 and the 1997 RWJF/RAND Employer Surveys),

[10] The NEHIS was conducted by the National Center for Health Statistics (NCHS) in partnership with the Agency for Health Care Policy and Research (AHCPR) and the Health Care Financing Administration (HCFA), and administered by WESTAT. The MEPSIC was conducted by AHCPR and administered by the U.S. Census Bureau.

but because of the geographical focus of the 1993 survey, they contain an overlap of only ten states between the two years.[11] The response rates for MEPSIC and NEHIS are high (approximately 70%) perhaps reflecting the confidence employers place on the federal guarantee of confidentiality.[12] However, these confidentiality rules come at a price in terms of the scope of research that may be performed across different data sets and require a unique approach to regression analysis, as I will explain below.

In order to convey the similarity between the MEPSIC and the NEHIS, I compare the two surveys' estimates of the fraction of all U.S. private-sector establishments offering health insurance, the fraction who self-insure at least one plan conditional on offering insurance, the fraction of workers who are offered health insurance, fraction actually obtaining health insurance through the employer, and the real 1996 dollar value of premiums and employee contributions. Estimates from the MEPSIC and the NEHIS appear closer in some aspects than others, although I expect some differences due to the different reference periods. For example, the fraction of establishments that self-insure at least one plan has risen by almost x percentage points when comparing the MEPSIC to NEHIS, although the fraction of establishments offering health insurance is approximately the same.[13] Thorpe and Florence (1999) also compare the two surveys' estimates by firm-size and industry and conclude that the two studies are very similar. Sommers (1999) notes that the NEHIS sampling design, definition of sampling strata etc. were used in the

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[11] The RWJF Employer Surveys contain data representative of ten states in 1993 and 12 states in 1997, although the 1997 sample was supplemented with approximately 2,000 firms from other states to produce nationally representative numbers. At the present time, only the 1993 data is available to the public. The NEHIS and MEPSIC are designed to produce state-level estimates of 50 and 40 states respectively, and the MEPSIC sample was supplemented with about 5,000 observations from the remaining 10 states to produce nationally representative data.

[12] For other differences between the NEHIS and the 1993 RWJF Employer Survey, see Hing, Poe and Euller (1999).

[13] This table is still under construction pending approval from Census. Because the MEPSIC is a new survey for the Census Bureau, rules have not yet been established about the nature of descriptive statistics that I may include in this paper.

MEPSIC with some modification.[14] A comparison of the two survey instruments also attests to their similarity.

However, there are some differences; while I do not expect them to affect the analysis, they should be noted. One difference is in the sampling frames used. While the NEHIS used the Dun and Bradstreet index of establishments to identify the universe of all firms from which to sample, MEPSIC used the Census Bureau's own Standard Statistical Establishment List (SSEL). The Dun and Bradstreet list counts all establishments that existed at any point in the year (a flow) while the SSEL counts only establishments alive at a particular point in time (a stock). Another difference is in the method of data gathering. The NEHIS relied on a telephone prescreener, a computer assisted telephone interview with questionnaires mailed out only when respondents refused to participate over the telephone. The MEPSIC was also initiated with a telephone prescreener but the main survey was mailed out in hard copy. A telephone follow-up was conducted in both cases for respondents providing conflicting data or missing crucial fields.

IV. Method

Although states' experiences with small-group reform span this entire decade, the focus in this paper is the 1993-1996 period during which a substantial amount of legislative activity occurred. Table 1 shows the reforms taking place in various states between these two years. The three possible reform circumstances a state can take are 'Full Reform', 'Partial Reform' and 'No Reform' as explained above. States that do not change their reform status appear along the main diagonal and form a control group. While no state reversed the course of reform between these two years, three states moved from none to partial reform, 12 states moved from none to full reform and 11 states moved from partial to full reform.

If state reform occurred as a truly natural experiment and states otherwise identical, the impact on small employers could be computed by comparing the average outcome of small-firms in reform states to those in non-reform states. If other state level differences exist and do not

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[14] For both surveys, sampling strata were "defined by the State in which the establishment is located, the size of the establishment, and the size of the firm that owns the establishment". (Sommers, 1999, p.2) However, while the NEHIS essentially over-sampled large firms, the MEPSIC over- sampled small firms.

change over time, we could compare the difference in outcome over time for small firms in reform states to the difference in outcomes over time in non-reform states. However, if states that reform change in other ways than states that do not reform, this estimation method may incorrectly attribute these changes as arising from reform. Since it seems plausible that large firms are also affected by some of these other insurance market forces but not by small-group reform itself, I use information on large firms to purge some extraneous influences from the estimated impact of reform. That is, I compare the change in outcome for small firms in reform states to change in outcome for large firms in reform states and then compare this to the similar estimate for non-reform states. The resulting 'difference-in-difference-in-difference' (DDD) estimator allows me to isolate effects due to state reform (see Gruber, 1994 for a good example of this approach, and Besley and Case, 1999 for the inadequacies of simple difference techniques.) This DDD estimate is calculated through a regression equation of the following form.

$$[1] \quad P_i = \beta_1 + \beta_2 X_i + \beta_3 C_i + \beta_4 S_i * F_i * POST_{it} + \beta_5 S_i * P_i * POST_{it} + \beta_6 A_i + \beta_7 Y_t + \beta_8 S_i + \beta_9 A_i * Y_t + \beta_{10} A_i * S_i + \beta_{11} Y_t * S_i + \epsilon_i$$

The left hand variable is an outcome of interest such as the premium charged by insurers to a firm *i*.[15] X stands for establishment, firm and workforce characteristics, C stands for insurance policy characteristics, F_i stands for whether the state ever has full reform and P_i stands for whether the state ever has partial reform, $POST_{ijt}$ stands for whether reform type *j* (where *j*=1 stands for full reform and *j*=2 stands for partial reform) is effective in state *i* in time *t*, A stands for state fixed effects, Y stands for time fixed effects, and S is an indicator for a small firm. While most states defined a small firm as one with fewer than 25 full-time workers, some used 50 full-time workers

[15] Other dependent variables in this study include the employee's contribution to coverage, whether the firm offers health insurance, the fraction of all workers covered, the fraction of all workers eligible, the fraction of eligible worker who accept coverage, whether an insured firm offers an HMO policy or a self insured policy, and whether they experience certain underwriting practices.

as a cutoff.[16] Because the 25-50 worker firms are in the treatment group in some states and in the control group in others, I eliminate them from my analysis and define a small firm as one with fewer than 25 full-time workers. Respondents from Hawaii are also excluded from the sample because of that state's employer mandate to provide health insurance.

A similarity between the two health insurance data sets used here which makes this project a challenging one is the strict confidentiality of responses. While the NEHIS is primarily accessible at the National Center for Health Statistics, the MEPSIC is available only at the Census Bureau's Center for Economic Studies. Although I have access to both surveys, they are intended to be used independently at their respective sites 25 miles apart. This restriction poses a barrier to regression analysis on the combined data, since they cannot be read simultaneously into a statistical program. Despite this limitation, I am able to compute regression coefficients using the basic properties of the matrix algebra of the OLS estimator, while still abiding by confidentiality protocols.

As an illustration, consider the simple case of regressing Y on X, where X and Y both contain data from 1993 and from 1996. Let X_{96} represent the sub-matrix consisting of 1996 data and Y_{96} the sub-vector of 1996 data.

$Y = X\beta + \epsilon$ *can be written as*

$$Y_{93} = X_{93}\beta + \epsilon_{93}$$

$$Y_{96} = X_{96}\beta + \epsilon_{96}$$

which is

$$\begin{vmatrix} Y_{93} \\ Y_{96} \end{vmatrix} = \begin{vmatrix} X_{93} \\ X_{96} \end{vmatrix} \beta + \begin{vmatrix} \epsilon_{93} \\ \epsilon_{96} \end{vmatrix}$$

The solution for $\hat{\beta}$, $(X'X)^{-1}(X'Y)$ *can be written using partition format as*

$$\hat{\beta} = = [X_{93}' X_{93} + X_{96}' X_{96}]^{-1} [X_{93}' Y_{93} + X_{96}' Y_{96}]$$

[16] According to national estimates available for 1993, about 80% of all workers work full time (NCHS, 1997 Table 16). A firm with 25-50 full-time workers may thus correspond to a firm with approximately 30-60 total employees assuming that the distribution of the percent of full-time workers does not vary substantially by firm size.

Notice that this solution does not require either of the X or Y matrices to be in their original form. The use of cross-multiplied terms such as $X'_{93}X_{93}$ helps maintain the confidentiality of survey responses.

Typically, we would need the following

$$\Sigma_i(Y_i - \bar{Y})^2$$

to construct the variance-covariance matrix for the estimated coefficients, but since that is not possible here (because of the strict confidentiality of any statistic containing Census micro data), I do the following equivalent calculation.

$$\begin{aligned}\sum_{i=1}^{T} \hat{\epsilon}_i^2 &= (Y - X\hat{\beta})'(Y - X\hat{\beta}) \\ &= (Y_{93}'Y_{93}) + (Y_{96}'Y_{96}) - 2\hat{\beta}'(X_{93}'Y_{93} + X_{96}'Y_{96}) + \hat{\beta}'(X_{93}'X_{93} + X_{96}'X_{96})\hat{\beta}\end{aligned}$$

The regression coefficients and their standard errors can be computed by taking the cross-product matrices from the first location to the second. To compute the adjusted R^2 of the regression, I could do the following where Y_D refers to the de-meaned values of the dependent variable.[17]

$$\bar{R}^2 = 1 - \frac{\hat{\epsilon}'\hat{\epsilon}/(T-K)}{Y'M^0Y/(T-1)}$$

where

$$M^0 = I - (\tfrac{1}{T})ii'$$

Then $\quad Y'M^0Y = (Y_{93D}'Y_{93} + Y_{96D}'Y_{96})$

In addition to computing coefficients and standard errors, I also construct F tests of restrictions

[17] This is work in progress.

on coefficients using matrix algebra.

$$F = \frac{(R\beta - q)'[\hat{\sigma}^2 R(X'X)^{-1}R']^{-1}(R\beta - q)}{J}$$

(where (Rβ-q) represents the restrictions to be tested) can be calculated in a straightforward manner once $(X'X)^{-1}$ and an estimate of σ^2 have already been calculated for producing coefficients and standard errors. However, it is important to note that the use of matrix algebra cannot accomplish other tasks such as corrections for heteroskedasticity unless the entire β vector can be transferred from the Census Bureau back to NCHS. [18]

V. Results

Descriptive Statistics

I construct my sample by excluding mid-sized employers[19] and employers in Hawaii from both the MEPSIC and the NEHIS. I am left with over 50,000 private sector establishments across the two years. and Table 3 I provide detailed descriptive statistics for both data sets.[20] In Table 3a I list mean characteristics for small and large establishments separately, while Table 3b contains information about health insurance plans for those establishments offering health insurance split by firm size.[21] These show that the percent of employers offering health insurance.....(I'll write this

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[18] To see whether rating reforms lead to reduced dispersion of premiums in the small-group market, I would also need to take the full set of coefficients from the premiums model out of CES to NCHS and compute the fitted residuals for both samples. I would then regress the squared residuals on the variables I had before. Because of restrictions on removing regression output from CES, I have not conduct this part of the analysis although I will requesting permission to do so.

[19] The language in most statutes defines a small firm in terms of the number of full-time workers. However, the surveys I use record only total firm size. Because 80% of all workers at the typical firm work full-time, I will assume that firms with 30 to 60 total employees will have between 25-50 full-time workers, and I exclude these firms from my analysis.

[20] Currently, the tables only contain NEHIS statistics.

[21] In the MEPSIC tables, some information is only given for the universe. Rules governing the release of descriptive statistics from MEPSIC do not permit me to these numbers in any more detail than this.

later once I have all the descriptive tables assembled). About 86 percent of all employers who offered health insurance in 1993 offered only one plan (NCHS, 1997). For those employers offering more than one plan, I retain only the plan with the most number of enrollees. Small-employers offering self-insured plans are deleted from the analysis because the Employee Retirement Income and Security Act of 1974 (ERISA) allowed these plans to avoid state regulation.[22] All premium and deductible numbers are shown in real 1996 dollars using the Bureau of Labor Statistics Consumer Price Index (CPI) for all goods.[23] The numbers in Table 3b show that plans offered by small firms are.....(to be completed later when tables are released).

I estimate the various regression models first on the whole sample and then according to an approximate ranking of the medical risk-level of a firm. Because health insurance outcomes are determined to a large extent by firm size, I control for the number of employees at the firm by including linear and quadratic terms. I also include a dummy variable for whether the firm has fewer than 30 workers because that is the definition of a small-firm used in this paper. If this were not included, then any effect of being an under 30-worker firm that is not captured by the linear and quadratic terms in firm size would be captured by the three-level variables that estimate the impact of reform, S*F*POST and S*P*POST in equation (1).

I expect insurance outcomes to differ by industry and I adjust for this with ten industry indicators.[24] A dummy variable indicating the presence of any unionized workers at the establishment allows for union-negotiated benefits to differ from situations where employees have less influence. Additional employer-level control variables include the age of the firm and its square, the fraction of low-wage workers (defined as those earning below $6.50 an hour in 1996

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22 Because of the possibility that the decision of a small firm to self-insure may be influenced by reform, I do not include these observations in the control group.

23 As an alternative, I used the Medical Care CPI but found that the key regression results were virtually identical. Between 1993 and 1996 the overall CPI rose by 8.5% while the medical care index rose by 14.5%

24 These are: agriculture, forestry and fishing; mining, oil and gas extraction; construction; manufacturing; transport, communication and utilities; wholesale; retail; finance; business and entertainment; and professional services.

and below $5/hr in 1993) and fraction of high-wage workers (defined as those earning above $15 an hr in 1996 and 1993).[25] Since premiums can be though of as the prices of different services multiplied by the quantity of services covered, it is important to control for the plan characteristics when considering the impact of reform on premiums and employee contributions. I use the type of plan (HMO or conventional or mixed), whether the plan is self-insured, the amount of the total deductible, whether there is a lifetime maximum benefit and its amount, the co-payment required for outpatient treatment, and the coinsurance rate for inpatient and outpatient treatment to proxy for the quantity of coverage provided by the plan. Additionally, I include 14 indicators for specific covered services such as outpatient prescription drugs (see Table 3b for a full list). When explanatory variables contain missing values, I create separate indicator variables which takes a value of 1 for a valid number and 0 for a missing value, and I replace missing values by zero.

Regression Results

The sampling structure followed in both surveys produced non-random samples, thus it is necessary to use weights when computing population estimates of descriptive statistics. Both files contain an original sampling weight for each observation which is the inverse of the probability that a certain establishment was selected for the survey, as well as a final sample weight which is higher than the original weight if there are any similar observations that did not respond to the survey. In addition, weights are adjusted to align the population estimates of these surveys with some other benchmark, a process known as 'post-stratification'. By multiplying the establishment sampling weight by the number of employees at the establishment, statistics can be computed to reflect the population of workers. By multiplying the establishment weight by the number of employees enrolled in health plans, statistics can be made to reflect the population of workers who receive health insurance from their employers. Although the unit of observation is the establishment, the story that is told is one related to firm-size and perhaps the relevant weight would be one reflecting the firm to which the establishment belongs. Given the choice of sampling

[25] In general, it is possible that wages and health insurance outcomes are endogenously determined through a wage-fringe benefit trade-off. However, the fact that wage information is captured here through a distribution of workers into three wage categories lessens this concern. When the models are re-estimated excluding wage variables, the main results do not change.

weights available, it is unclear which weight should take precedence in this analysis.[26] From here on I concentrate on unweighted results partly because of their intuitive appeal to economists and partly because my sample consists of two surveys whose sample-weight construction techniques may differ in ways that I do not fully appreciate. However, this is a question I intend to follow in future work.

The regression results presented in Tables 4 onwards utilize the matrix technique explained above. Cross-product matrices are first computed using NEHIS data, taken to the Census Bureau and combined with similar cross products from MEPSIC. In Table 4 the coefficient on S*F*POST indicates the impact of full reform and S*P*POST is the impact of partial reform. The omitted category is 'no reform', so these coefficients show the effect on small-firms of having partial or full reform compared to having no reform. Given that full reform is a stronger combination of restrictions on insurers than partial reform and is thus expected to have a greater impact on outcomes, in Table A of the Appendix I re-estimate the regression models in Tables 4 through 6 for the whole sample to study the impact of having full-reform against all else (including partial reform and no reform states in the control group). Sample means, the number of observations in a regression, (*and soon, adjusted* R^2) and the F-statistics to test the null hypothesis that the coefficient on S*F*POST and S*P*POST are jointly equal to 0 are reported in separate columns. The coefficients in Tables 4a through 6a and Table A of the Appendix are unweighted, while Tables 4b through 6b shows the coefficients from regressions using different sampling weights.

The first question I pose is whether reforms affected the premium that a small employer pays for a single health insurance policy.[27] Ideally I would require the outcomes such premiums and percent of employees insured at the firm level, but the available data only contain these measures at the establishment level. Because almost all small firms have only one establishment, I

[26]and why they produce different estimates in some cases.

[27]

For all dependent variables I use unimputed data although I include imputed data on a variable that appears only as an explanatory factor. When the imputed value is missing, I create a dummy variable which takes a value of 1 if the data are valid and 0 otherwise, and include this along with the actual variable itself.

refer to a small establishment and a small firm interchangeably in the following discussion.[28] The results in Table 4 indicate that the impact of full reform is positive but that the magnitude and statistical significance depend on the sampling weight used. Establishment-weighted results indicate that full reform increased premiums by $13.10 (with a standard error of 5.28), and that both unweighted and employee-weighted results show a statistically insignificant increase of about $7. Partial reform also appears to have increased premiums by $10.33 (with a standard error of 6.12) when considering the establishment-weighted results, but have had statistically insignificant effects of between $1.52 and -$2.3 when looking at employee-weighted or unweighted results. Table A indicates that full reform has caused a statistically significant increase in premiums of $8.4. Given that partial reform does not constrain insurers the way that full-reform does, it is puzzling that the impact of partial reform is as strong as it is in establishment weighted results.

Turning to employee contributions in the second half of Table 4a, it appears that full-reform increased the payment required of employees by $6.33 (with a standard error of 2.99), a number that is statistically significantly different from zero at the p=0.10 level. The impact of partial reform is much smaller in magnitude and statistically insignificant. This increase in employee contributions is 78% of the coefficient showing the increase in total premiums as a result of full reform ($5.67/$7.26) and statistically significant when considering the unweighted results. The establishment-weighted results indicate that less than that fraction was passed on to employees ($6.33/$13.10), while employee-weighted numbers indicate that more than this was passed on ($8.11/$6.47). According to Table A, 68% of the increase in premiums were passed on in the form of higher employee contributions ($5.7/$8.4), and both the numerator and the denominator are statistically significant.

An increase in the cost of health insurance may force an employer to stop offering this benefit if the employer is unable to pass this cost on to employees. Given the results in Table 4, I

[28]In 1993, for over 90% of the small firms, the firm and the establishment are the same.

would not expect reforms to affect the employer's decision to offer coverage by much.[29] The results in Table 5 indicate that both full and partial reforms have almost no impact on whether an employer offers health insurance, although the coefficient on full reform is negative (-0.01 with a standard error of 0.02), as it in Table A as well. The coefficient on partial reform is 0.00 with a standard error of 0.02. Employee and establishment-weighted results also do not indicate much impact. The second set of columns in Table 5 considers the effect of reform on the percent of workers at the establishment who are eligible to receive health insurance.[30] The coefficients indicate that both full and partial reforms have exerted negative influences on unweighted, employee-weighted and establishment-weighted results, but that the standard errors are large and the t-statistics never rise above 1.4.

If premiums increase, and if employers are able to pass much of this cost on to employees, I expect that coverage and take-up rates would fall.[31] Table 6 suggests that this is a possibility. The unweighted impact of full reform on the percent of workers covered by health insurance at the establishment is a drop of 2.25 percentage points (from a base of ..), with a standard error of 1.5.[32] The impact of partial reform is a drop of -0.95 percentage points with a standard error of 1.55. While the employee-weighted results are weaker than the unweighted results, the

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The decision to offer health insurance is a dichotomous variable, and as such a limited dependent variable method of estimation would be most appropriate. However, matrix algebra methods are not capable of replicating maximum likelihood estimation, and a linear probability model is expected to provide a good approximation. Also, the inter-related nature of the dependent variables I consider make a seemingly unrelated regression estimation desirable, but impossible given the constraints in this particular situation.

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Eligibility is defined as the number of workers at the establishment considered eligible for health insurance divided by the number of all workers at the establishment.

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Coverage is defined as the number of workers at the establishment who accept coverage over the number of workers at the establishment, and take-up is defined as the number of workers at the establishment who accept over the number of workers who are eligible for health insurance. Eligibility is usually determined by the numbers of hours worked (usually only full-time workers are eligible) and the length of time an individual has worked at the firm.

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For establishments not offering health insurance, the fraction of workers covered and eligible are set to 0.

establishment-weighted results are stronger. They indicate that full reform decreased the percent of workers covered by health insurance about -4.27 percentage points, while partial reform had an impact of -4.12 percentage points, and these results are jointly statistically significant. Full reform had a statistically insignificant but negative effect on take-up rates (defined as the fraction of workers offered insurance who accept the offer) in both weighted and unweighted models.

Altogether, the signs and magnitudes of coefficients indicate that the average small firm experienced an increase in premiums, the major share of which was passed on in the form of increased employee contributions, and the insurance coverage rate among workers may have declined although the firm's decision to offer health insurance appears unaffected. However, these results are often statistically insignificant and an equally plausible interpretation is that full and partial reforms have had little impact on the average small firm. Recall that theory predicts that full reform may have had an ambiguous impact on the average small firm, but that the impact should vary by risk level.

In order to examine whether low and high-risk firms were differentially affected by regulations, I would ideally need information about specific health conditions of workers. I know of no employer survey containing medical information, and although the MEPSIC collects some demographic information, the NEHIS has virtually none that would allow me to distinguish firms by medical risks. However, a 1991 survey asked insurers to report the four-digit industry code of firms that they red-line.[33] By identifying red-lined firms in my sample based on their four-digit industry codes, I can define a high-risk group that constitutes about 15% of my sample in industries such as florists, explosive manufacturers and hair-dressers.

Another less precise strategy to identify risk groups is to impute demographic characteristics for three-digit level industries using the 1990 5% Public Use Micro Sample (PUMS) from the Decennial Census.[34] I first identify demographic groups that are likely to differ

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[33] This list was collected as part of a survey of insurers, the results of which appear in Zellers, McLaughlin and Frick (1992). I am grateful to the authors for sharing this list with me.

[34] It is important to note that states differed in the rules by which insurers could set rates, but that about half of the states with rating reforms limited the use of at least one key demographic factor such as gender, age, etc.

in their use of medical services from the MEPS Household Survey of 1996. Never-married males under the age of 35 years are between two to five times less likely to see a doctor or be hospitalized than are married women of childbearing age with children. I then use the 1990 PUMS to compute the percentage of private sector full-time workers in a given state-by-three-digit-industry cell who have certain demographic characteristics, and I match this information to the establishment data sets using the same state and industry cells.[35]

This method of imputing demographic statistics is clearly a less precise method of identifying the risk of a firm than the previous way of selecting red-lined firms. Using the 1990 PUMS to infer the demographic characteristics of industries produces a statistically accurate estimate of the demographic characteristics in a certain state by industry cell only if the average demographic characteristics in industry-state cells have not changed from 1990 to 1996. Because of the changing pattern of employment from 1990 to 1996, especially among married women, I do the following exercise to get a sense of this method's accuracy. The MEPSIC does not ask for the fraction of young never married males or the fraction of young married women with children employed by an establishment, but it does ask for the percent of female employees in the establishment's workforce. I construct an imputed value for the percent of women at the industry-state level using the PUMS, and I compared this to the reported figure in the MEPSIC and found that the two measures had a correlation coefficient of 0.61.[36] Albeit imperfect, this method gives some indication of the relative risk level of a firm.

I next explain the method by which I classify employers belonging to certain state-industry cells as high or low-risk. In order to do this, I first observe the distribution of imputed demographics in each survey. The 75th percentile value of the imputed percent of male, never-married workers under 35 years of age (henceforth referred to as low-risk for simplicity) is x in

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[35] I thank Kim Bayard for providing me with the cross-walk between the Census and SIC three-digit industry codes.

[36] The mean of the imputed value is 0.34 and the mean of the reported value is x.

the NEHIS and x in the MEPSIC.[37] I classify an employer as being low-risk if the imputed percent of low-risk workers in that state-by-industry cell is above x in the NEHIS or x in the MEPSIC. I identify state-by-industry cells dominated by married women employees under 41 years of age with young children (henceforth referred to as high-risk employers for a simplicity) in a similar way as being a high-risk group of employers.

If a certain state-by-industry cell has 50 percent high-risk and 50 percent low-risk workers, the method above will classify that cell as both high-risk and low-risk. In order to avoid this possibility, I consider an alternative decision rule. I classify risk groups by using the 90th percentile values as the dividing line, and also by requiring that a low-risk group have values above the 75th percentile for the share of low-risk workers and values below the 25th percentile for the share of high-risk workers.

The second through fourth rows of Tables 4-6 present the results for the six different dependent variables by high and low-risk firms. In Table 4a the unweighted OLS coefficients move in the direction one would expect (that the high-risk small firms experience premiums reductions and the low-risk small firms experience premium increases relative to the average small firm) but these effects are generally statistically insignificant. The only statistically significant result for premiums by risk level is that partial reforms reduce reduced the premium for high-risk firms by close to $29.[38] The next set of columns show that full reform has increased the employee contributions at low-risk firms by $15.77 (with a standard error of 6.61), while partial reform has decreased the contribution for high-risk firms by between $13.8 and $10.1. These results are all

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Labeling these particular groups as high and low risk is not ideal for several reasons. For example, insurers in San Francisco may consider firms with many young never-married male under 35 yrs as a high-risk firm because of the high concentration of HIV positive cases in that demographic group in that region. I thank an anonymous referee on a previous paper for this point.

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This result is troubling because partial reforms are expected to have less impact than full reform, which displays a coefficient of X and a standard error of x here. One possibility is that insurers saw that partial reform was soon followed by full reform and acted on that expectation. Often there was a lag of over one year between the introduction and the effective dates of legislation. Thus the cutoff date I used (the start of 1993) may be too conservative, and some partial reform states should be coded as full-reform states if full reform was effective anytime during 1993. When I ran alternative models using cutoff dates at the middle and the end of 1993, I found that this reasoning does not provide an explanation for the observed results.

statistically significant at the p=0.1 level.

Looking at Table 5, it appears that although all coefficients are statistically insignificant, their magnitudes and signs move in directions one would expect: reform increased the proportion of high-risk employers offering health insurance and lowered the proportion of low-risk firms offering insurance. In terms of the percent of workers eligible for coverage, the same story holds as for the decision to offer health insurance. Full reform displays a negative 4.06 percentage point impact for low-risk firms, although this is again statistically insignificant. In Table 6 the impact on the percent of workers covered by insurance is statistically insignificant, although the magnitudes and directions move once again in the direction I expect. The coefficient on full-reform for red-lined small firms is a 2.00 with a standard error of 3.28 while the coefficient for low-risk firms is -4.7 with a standard error of 3.11.[39] The take-up rates also tell a similar story.

When considering these regression results, two things must be kept in mind. First, although the coefficients tell a compelling story that reforms affected insurance outcomes in predictable ways, lack of statistical significance means that one could also conclude that reforms have had no strong effects. The second fact to bear in mind is that the impact of reform is identified from a three-level interaction term, and that the sample may need to be much larger to produce precise estimates. In Simon (1999b), the estimates were precisely calculated from a similar DDD method with over 200,000 observations on individuals, where the number of observations in the treatment category was x. The sample size in this paper is 50,000 at most, with only x observations falling into the treatment category.

In addition to the DDD estimates in Table 4-6, I also show the DD estimates for the impact of reform in several ways. Table 7a is divided into two sets of columns, one showing the results for large firms and the other for small firms separately. Large employers have experienced no effects at the same time as small-group reform, although the coefficient on the premium

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[39] To investigate the effect of reform by risk level further, I classify the risk level of a firm by requiring a low-risk firm to have more than the 75th percentile for the share of low-risk workers and fewer than the 25th percentile value for high-risk workers and vice versa for high-risk firms. Although sample sizes diminish and standard errors rise, the coefficients almost always intensify in the direction I expected: that high-risk benefit while low-risk fare worse.

regression is negative but statistically insignificant.[40] In Table 7b I show the DD estimates for 1993 and 1996 separately (not done yet). These show that the effects are smaller but of the same sign when compared to the DDD results.

A highly persuasive test of the robustness of results using NEHIS and MEPSIC would be estimate these same equations using two other data sets of employers from 1993 and 1996 and to see whether results correspond. Although data sets of this nature are not available, a robustness test can be done with data from the public-use RWJF 1993 Employer Survey for 10 states combined with data on the same states from the MEPSIC.[41] I create a data set matching this survey to data on the same 10 states from the MEPSIC at the Census Bureau, and I estimate equations similar to (1) above using conventional means (i.e. regression procedures pre-programmed in SAS) and report the results in Table 8. Since these results are not comparable to those from the NEHIS/MEPSIC regressions using all states, I re-estimate the matrix regressions for the NEHIS/MEPSIC using the 10 RWJF states. In both cases I obtain negative and statistically insignificant results, and the coefficient appears larger than when all 50 MEPSIC/NEHIS states were used in Table 5.

State regulators intended to promote more equitable insurance markets in addition to improving the rate of insurance coverage. While the analysis above studied the impact of reform on the later, the effectiveness of reform should also be judged in terms of whether they reduced costly underwriting practices. In Table 9 I look at the impact of reform on whether a plan is allowed to exclude certain individuals for the life of the policy or whether the plan may impose a waiting period during which pre-existing illnesses are not covered.[42] State regulations enacted under full reform made a more concerted effort to stem these practices than regulations falling

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[40] This is consistent with the suggestion that large firms control for other changes in the insurance market structure, such as an increase in competitiveness and a drop in prices following increase managed care penetration.

[41] The RWJF survey contains health-insurance related information for approximately 20,000 employers in New York, Minnesota, New Mexico, Washington, Vermont, Oklahoma, Oregon, Florida, North Dakota and Colorado.

[42] Disclosure considerations may not permit me to regress these two dependent variables separately.

under partial reform. The results in Table 9 indicate that full reform decreased the likelihood that these practices occurred at all by a statistically significant 5 percentage points (from a base of x) while partial reform had no impact.[43]

A new question that arises from these results is whether employers may have adjusted to a possible increase in premiums by switching to lower quality health insurance or self-insuring. While there are many ways to measure a change in quality of health insurance, I chose the type of plan offered as an indicator of quality.[44] HMOs are often thought of as a cheaper alternative to conventional plans. The results in Table B of the Appendix are from a DDD model where the explanatory variables are the same as in equation (1) but the dependent variable is whether the firm offers a managed care product or not.[45] It appears that small firms may indeed be more likely to offer a managed care plan in states that reform, a suggestion that has been advanced elsewhere (Buchmueller and Di Nardo, 1999, and Buchmueller and Jensen, 1998). Reforms are correlated with the probability that a small firm offers a managed care product by almost 5 percentage points. However, when I compute the DD estimate for large firms (not reported in the table) this impact is also positive and statistically significant suggesting that managed care penetration may be occurring at the same time as reform and that small employers are drawn to these plans more than large firms, but that this may not be caused by reform. Looking at whether a firm self-insures any plan, I find that the DD estimate for small firms suggest the correlation is positive and statistically significant, although the DD estimate for large firms is negative and statistically significant. Although no conclusion should be drawn from this analysis about whether this is an effect of reform or not, it is an issue worth further probing.

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[43] The reforms mostly attempted to reduce the intensity of these practices, for example, by limiting the period during which pre-existing conditions are excluded. The information on the extent of the exclusions is not available for both surveys.

[44] In future work I plan to look at whether a certain benefit like prescription drugs is offered as a measure of generosity.

[45] I look at whether the employer offers a managed care product at all, not just whether it is the most popular plan that is offered.

In summary, the regression coefficients and signs appear to indicate that full-reform caused premiums to rise for the average small-firm. Although theory does not make direct predictions on the movement of employee-contributions, the fact that they also increase by almost the same magnitude provides an internal consistency check of whether the results obtained are plausible, and is also consistent with the notion that employees ultimately bear the cost of fringe benefits. It appears that the small employer's decision to offer health insurance was not affected by much as a result of reform, although the percent of actually receiving health insurance coverage falls by close to 2 percentage points. The percent of workers offered insurance also appears to fall, but by a smaller magnitude.

Results for separate risk groups also appear to support the hypothesis that full-reform would increase premiums and reduce insurance coverage rates for low-risk small employers while having the opposite impact on high-risk employers. However, not all results are as expected (for example, it is troubling that partial reform, and not full reform has caused bigger changes in premiums by risk groups in some cases), and are often statistically insignificant. Nevertheless, the general sign and magnitude of coefficients indicate that these results are consistent with adverse selection behavior.

V. Conclusion

This paper uses a large data set on over 50,000 employers spanning all states and three years as well as legislative data on state reforms from a primary investigation to investigate the impact of state regulation concerning the health insurance market for small employers. These insurance reforms aimed to improve conditions for small-employers by encouraging insurers to pool the risks of their clients. While state policy makers hoped that these regulations would help small employers, I hypothesize that reforms could lead to losses for the low-risk population, gains for the high-risk population and perhaps a decline in insurance outcomes for the average small employer because of adverse selection behavior.

I use a unique method of conducting analysis with confidential data sets using matrix algebra, and an identification approach which compares the difference in health insurance outcomes among small firms in states that reformed before and after reform while controlling for

several other factors that could incorrectly be attributed to reform. I find that full-reform appears to increase premiums and employee contributions for the average small firm by $7 to $8 in unweighted regressions, and that the percent of workers covered by the average small employer's health insurance policy declined by close to 2 percentage points. Given that employers are able to adjust to the impact of reform on premiums by increasing employee contributions or tightening eligibility conditions for health insurance, it is not surprising that the decision to offer health insurance does not appear to be affected substantially by reform. The good news, however, is that stringent regulations have succeeded in reducing the incidence of costly underwriting practices.

As in Simon (1999b), this study also finds evidence to suggest that low-risk small firms were made worse off by reforms while high-risk small firms were not harmed. The premiums, employee contributions, the percent of workers covered by health insurance and the employer's decision to offer health insurance regressions all display signs and sometimes statistically significant coefficients that are consistent with theory. Moreover, the impacts of small-group reform estimated in this paper are extremely consistent with results in Simon (1999b). The impact of full reform on the probability that the average small-firm worker received insurance from his/her employer in Simon (1999b) was a statistically significant drop of two percentage points. Here, the impact of full reform on the percent of workers given health insurance by small firms is a statistically insignificant drop of 2 percentage points. The impact of full-reform on low-risk individuals at a small firm (defined in a similar way) in Simon (1999b) was a statistically significant drop of seven percentage points. In this paper, the impact of full-reform on the percent of workers given health insurance by a small low-risk employer is a statistically insignificant drop of five percentage points.

Although the results appear plausible and robust in many ways, several issues warrant further investigation. One is that results appear sensitive to the choice of sampling weights used regressions. Because of the intuitive appeal of unweighted analysis to economists and because the sampling techniques between the two data sets are not identical, I concentrate on unweighted results and present weighted results for comparison. The other is that partial reform, which should have a weak impact since insurers are still allowed to deny health insurance to clients under this regime, sometimes appears to influence outcomes to an equal or greater degree than full-reform.

The third is that most results are statistically insignificant and one could also interpret them to indicate that small-group reforms have had very little impact.

In conclusion, the contributions of this paper are threefold. First, I present new evidence on the impact of small-group reform on small employers. Prior to this study, no econometric evidence existed about what small-group reforms have done to the price of health insurance. Second, I gathered reliable data on state reform though direct means; through contact with state officials and from state legislative records and I was able to avoid inconsistencies apparent in secondary sources. Third, I use a unique method to analyze confidential data without which no econometric comparisons could be made between the MEPSIC and the NEHIS. Surveys administered under strict guarantees of confidentiality are invaluable for researchers, yet new techniques such as the one developed in this study are crucial if these data are to be used in an effective manner.

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Table 1:Small-Group Reform Changes, 1993-1996

1993⇓ 1996⇒	Full Reform	Partial Reform	No Reform
Full Reform	CT IA KS MA NC OH RI TN VT WY ID MT		
Partial Reform	DE FL LA ME ND NE NH NM OK SC SD	AR GA IN OR WI WV	
No Reform	AK CA CO KY MD MN MO NJ NY TX VA WA	IL MS UT	AL AZ DC MI NV PA

Table 2: NEHIS and MEPSIC Comparisons

Variable	MEPSIC (1996)	NEHIS (1993)
Percent offering major health insurance #		
Percent of insured who self-insure at least one plan#		
Percent of workers offered ##		
Percent of workers covered ##		
Premium for single coverage		
Employee contribution for single coverage		

(Note: Descriptive Statistics are not yet available from Census)

Table 3a Descriptive statistics for the 1993, all establishments

	Small Firms			Large Firms		
Variable	N	Unweighted	Weighted	N	Unweighted	Weighted
Fraction offering	16608	0.42	0.40	15125	0.96	0.95
Firm size (# employees)	ditto	7.75	6.75	ditto	18147	12052
Agriculture, forestry and fishing		0.04	0.04		0.004	0.002
Mining, extraction		0.10	0.10		0.031	0.03
Construction and manufacturing		0.07	0.07		0.19	0.10
Transport, communication and utilities		0.035	0.033		0.08	0.07
Wholesale		0.07	0.076		0.07	0.095
Retail		0.23	0.22		0.24	0.28
Finance		0.07	0.07		0.10	0.15
Business and entertainment		0.16	0.16		0.08	0.09
Professional services		0.21	0.21		0.18	0.16
Union presence	15924	0.02	0.02	13123	0.13	0.08
Age of firm (yrs)	15818	20.4	19.2	14481	50.6	48
% low wage	15090	16.4	16.3	10891	9.9	12.1
% high wage	15014	17.4	18.84	10430	22.2	21

Table 3b Descriptive statistics for 1993, establishments offering insurance

	Small Firms			Large Firms		
Variable	N	Unweighted	Weighted	N	Unweighted	Weighted
Premiums	5791	190.13	195.4	10774	181.34	182.4
Self insured	-	-	-	10774	0.49	0.45
HMO	5791	0.12	0.14	10774	0.16	0.19
Mixed type	5791	0.35	0.36	10774	0.40	0.39
Total deductible	5157	335	336	10392	208	208
Maximum benefit	3897	4878100	4935922	9513	3973760	4374483
Copay outpatient	3369	4.6	4.76	5331	6.5	6.5
Coinsurnace,inpatient	5622	15.9	15.3	10417	14.2	14
Coinsurance,outpatient	4119	11.5	11.12	7126	14.6	14.5
Routine mammograms	4888	0.77	0.77	10262	0.78	0.78
Routine physicals	5302	0.63	0.63	10493	0.58	0.58
Routine pap-smears	5002	0.75	0.75	10353	0.76	0.75
Child immunizations	4374	0.66	0.67	10263	0.69	0.67
Well-baby care	4356	0.69	0.69	10299	0.69	0.67
Well-child care	4286	0.67	0.67	10160	0.63	0.63
Outpatient drugs	5485	0.77	0.76	10579	0.94	0.93
Routine dental care	5585	0.21	0.20	10587	0.36	0.34
Orthodontics	1091	0.43	0.44	3676	0.59	0.54
Nursing home care	4082	0.32	0.34	9461	0.51	0.52
Home health care	4211	0.55	0.54	9728	0.84	0.83
Inpatient mental health	4710	0.84	0.83	10328	0.96	0.95
Outpatient mental health	4319	0.73	0.71	10181	0.94	0.93
Alcohol/drug abuse care	4482	0.75	0.74	10227	0.93	0.92

Table 4a: Unweighted OLS Results (Coefficients and Standard Error) for Premiums and Employee Contributions.

	Premium					Employee Contribution				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	26,215	181.1	7.26	-2.3	2.1	28,052	32	**5.67**	0.23	2.6
	1		(4.88)	(5.16)		19		**(2.83)**	(2.98)	
Red-lined industries	4,284	189.2	5.00	-8.9	0.6	4,543	37	-4.38	**-13.8**	1.5
	69		(12.3)	(13.3)		65		(7.44)	**(8.07)**	
Above 75th percentile for	5,439	178.8	7.27	1.56	0.2	5,703	37.1	**15.77**	-3.11	**4.4**
% never married males under 35	72		(10.9)	(12.37)		67		**(6.61)**	(7.42)	
Above 75th percentile for	6,717	182.9	-11.7	**-28.70**	**5**	7,229	34.7	-5.4	**-10.1**	1.5
% young married women with kids	73		(9.5)	**(9.3)**		76		(5.96)	**(5.33)**	

Table 4b: Weighted OLS Results (Coefficients and Standard Error) for Premiums and Employee Contributions, Whole Sample.

	Premium (1)					Employee Contribution (19)				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	26,215	187.7	**13.10**	**10.33**	**3.3**	28,052	29.9	**6.33**	1.86	2.3
			(5.28)	**(6.12)**				**(2.99)**	(3.45)	
Employee weighted results	26,215	180.8	6.47	1.52	0.8	28,052	31.2	**8.11**	3.80	**3.4**
			(5.43)	(6.43)				**(3.12)**	(3.7)	

Table 5a: Unweighted OLS Results (Coefficients and Standard Error) for Decision to Offer and Percent Eligible

	Decision to Offer					Percent Eligible				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	50,485	0.66	-0.01	-0.00	0.2	47,149	51.6	-2.1	-0.11	1
			(0.02)	(0.02)				(1.7)	(1.76)	
Red-lined industries	8,839	0.6	0.05	0.053	1	8,311	42.2	2.11	4.93	0.7
			(0.04)	(0.043)				(3.96)	(4.12)	
Above 75th percentile for	12,229	0.56	-0.011	-0.001	0	11,445	39.4	-4.06	-0.71	0.7
% never married males under 35			(0.04)	(0.041)				(3.78)	(4.14)	
Above 75th percentile for	12,479	0.69	-0.006	0.013	0.2	11,438	52.4	-1.99	2.29	1
% young married women with kids			(0.03)	(0.032)				(3.48)	(3.2)	

Table 5b: Weighted OLS Results (Coefficients and Standard Error) for Decision to Offer and Percent Eligible, Whole Sample.

	Decision to Offer					Percent Eligible				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	50,485	0.53	0.00	0.01	0.1	47,149	42	-2.8	-2.3	1
			(0.02)	(0.02)				(2.11)	(2.4)	
Employee weighted results	50,485	0.85	0.016	-0.01	1.3	47,149	68.1	-0.87	-1.75	0.6
			(0.013)	(0.015)				(1.4)	(1.63)	

Table 6a: Unweighted OLS Results (Coefficients and Standard Error) for % of all Employees Covered and Take-up

	Percent Employees Covered					Take-up Rate				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Whole sample	47,598	42.9	-2.25	-0.95	1.1	46,641	51.9	-2.35	-0.8	1
			(1.5)	(1.55)				(1.72)	(1.8)	
Red-lined industries	8,428	32.1	2.00	2.13	0.3	8,253	43.7	3.84	3.61	0.5
			(3.28)	(3.4)				(4.15)	(4.3)	
Above 75th percentile for	11,625	31.1	-4.7	-2.7	1.1	11,375	41.5	-5.3	-3.5	1
% never married males under 35			(3.11)	(3.42)				(3.8)	(4.2)	
Above 75th percentile for	11,581	41.65	0.53	-1.06	0.2	11,287	51.9	1.54	-1.18	0.4
% young married women with kids			(3.03)	(2.9)				(3.63)	(3.46)	

Table 6b: Weighted OLS Results (Coefficients and Standard Error) for % of all Employees Covered and Take-up, Whole Sample.

	Percent Employees Covered (25)					Take-up Rate (132)				
	N	Mean	S*F*Post	S*P*Post	F	N	Mean	S*F*Post	S*P*Post	F
Establishment weighted results	47,598	34.7	**-4.27**	**-4.12**	**3.3**	46,641	41.6	-2.76	-1.26	0.8
			(1.8)	**(2.09)**				(2.15)	(2.45)	
Employee weighted results	47,598	57.7	-0.75	-1.25	0.4	46,641	70	-0.62	-2.31	1
			(1.33)	(1.5)				(1.41)	(1.64)	

Table 7: OLS Unweighted Results for Diff in Diff: Small Firms and Large Firms Separately

	Small Firms				Large Firms			
	N	Mean	F*Post	P*Post	N	Mean	F*Post	P*Post
Premiums	10,007	189.6	2.9	-3.4	16,208	176.1	-4.1	0.26
			(4.53)	(4.84)			(2.58)	(2.71)
Employee Contribution	10,191	28.3	**5.76**	-0.48	17,882	34.17	0.39	-0.96
			(2.78)	(2.97)			(1.42)	(1.48)
Decision to Offer	29,003	0.43	-0.014	-0.01	21,482	0.96	-0.006	-0.008
			(0.01)	(0.01)			(0.007)	(0.007)
Percent of Workers Covered	28,767	28.5	-1.21	-0.42	18,831	64.8	0.77	0.54
			(0.94)	(0.97)			(0.99)	(1.03)

Table 8: Unweighted Results for Decision to Offer for 10 State Sample

	MEPSIC and RWJF			MEPSIC and NEHIS		
	N	Mean	S*Reform*Post	N	Mean	S*Reform*Post
Whole sample	24,437	0.66	-0.03	10,163	0.65	-0.01
			(0.04)			(0.03)

Table 9: Effect of Reform on Underwriting and Exclusion Practices

	N	Mean	F*Post	P*Post
Any prex-or exclusion?	33,094	0.58	-0.05	-0.006
			(0.026)	(0.027)

APPENDIX

Table A. What happens when I look at Full-Reform vs all else? (Whole sample)

	N	Mean	S*Ref*Post
Premiums	26,215	181.1	8.4
			(4.2)
Employee Contribution	28,052	32	5.7
			(2.5)
Decision to offer	50,485	0.66	-0.01
			(0.0145)
Coverage rate	47,598	42.9	-1.78
			(1.29)

Table B : OLS Unweighted Results: Decision to Offer HMO Plan

	N	Mean	S*F*Post	S*P*Post
Whole sample	33,094	0.23	**0.049**	**0.04**
			(0.02)	**(0.02)**
Redlined establishment	5,237	0.23	-0.04	0.002
			(0.06)	(0.06)
Above 75th percentile for	6,795	0.24	0.048	0.024
% never married males under 35			(0.05)	(0.05)
Above 75th percentile for	8,493	0.20	0.004	0.055
% young married women with kids			(0.044)	(0.04)